

And where did Gertrude Schmeidler report her study finding that 90% of apparitions show agent motivation, whereas only 10% show percipient motivation? In the paper cited by Becker, Schmeidler says no such thing and in fact tentatively suggests the *opposite* hypothesis in connection with the case reported. Becker seems to have confused Schmeidler with E. P. Gibson (see p. 49).

I could go on and on with such examples, major and minor; but readers may be wondering why I have gone on *this* long. I believe that it can never hurt to remind ourselves, frequently and forcefully, that we sometimes need to be even more wary of well-intentioned friends and supporters of the field who misrepresent it than we do of not-so-well-intentioned critics who (in different ways) also misrepresent it. This caution is all the more needed when a book has all the appearance of a scholarly book and is published by a reputable university press, but is nonetheless unreliable. This raises in turn the question of how it happened that the publisher's readers approved this book in its present form. Presumably their job is to verify the accuracy of an author's statements and supporting references. If *they* do not know primary sources for the subject of a book, then they cannot function effectively, and the publisher should turn to more knowledgeable readers and reviewers.

Becker asks: "With such good evidence for survival, why does the scientific community still reject the idea of survival so often?" (p. 119). If books such as this serve as their only source of information about survival research, one need not wonder.

EMILY WILLIAMS COOK

Division of Personality Studies
Box 152 Health Sciences Center
University of Virginia
Charlottesville, VA 22908

PSYCHIC SLEUTHS: ESP AND SENSATIONAL CASES. Edited by Joe Nickell.
Buffalo, NY: Prometheus Books, 1994. Pp. 251. \$24.95, hardcover.
ISBN 0-87975-880-5.

This is an important book, for it well represents the position of severe critics of the use of alleged psychics by police and criminal investigators. Though editor Nickell writes in his introduction that the book is not a project of the Committee for the Scientific Investigation of Claims of the Paranormal (CSICOP), nor is it endorsed by CSICOP, the book's "task force was generally guided by the objectives of CSICOP" (p. 18). Many of the 11 contributors are prominently associated with Paul Kurtz's CSICOP, and the book is published by Prometheus Books, and so an

informal connection is obviously present. Since CSICOP has been heavily embattled by lawsuits alleging libel, Nickell's disclaimer may be present more for reasons of legal than intellectual disassociation. Because of the book's many references to probable fraud rather than just error by some of the psychics examined, Nickell's caveat may prove a wise one.

Though this collection is uneven in its contributions (some of them are excellent), it is well worth reading, especially for what it tells us about the mindset of these hard-line "skeptics" and the rhetorical devices they use to discredit and dismiss, when they are unable actually to disprove, the claims they examine.

Because substantial portions of this book are directed toward matters raised by Arthur Lyons and me in our book *The Blue Sense* (1991), and because I presumably was invited to review it in part for that reason, this review will unavoidably be a response as well as an appraisal. In his introduction, Nickell grossly distorts the position that we took in *The Blue Sense*. He mistakenly asserts that we use the term *blue sense* (which is police vernacular for a cop's intuition) simply as a way to "rename" psi (p. 16). In fact, we used the term *blue sense* precisely because we did not want to be concerned only with psi but because we also wanted a broader category that might include still mysterious but not necessarily paranormal processes. For example, when we wrote of Greta Alexander, we said: "She has something *interesting*, if not psychic, going for her. And that *something* is worth learning more about. We have called that special something the blue sense" (Lyons & Truzzi, 1991, p. 185). We used the term *blue sense* (as do cops who use the term) simply to refer to whatever is going on. This broader meaning has also been explicitly stated by me elsewhere.¹ As most reviewers of our book understood, Lyons and I clearly did not endorse the existence of psi (a point we made repeatedly).

Further, Nickell apparently agrees with psychologist Robert A. Baker, with whom Nickell has collaborated on other writings and whom he quotes here at length, saying that we are "pseudo-skeptics" who are "clearly out to convince all comers that 'the blue sense' exists and that in certain heads at certain times it is valid" (Nickell, quoting Baker, p. 17). This statement is rather ironic since our book refers to CSICOPers like

¹For example: "In our investigations, we felt it important not to prejudge possible mechanisms that might be involved in what ostensibly appeared to be psi episodes. When we heard one of our police informants refer to the intuitive abilities a cop acquires from experience as 'the blue sense,' we felt that this label for what may largely be unconscious pattern recognition could well be expanded to cover the wider phenomena we were interested in. Whether or not the ability of some people to produce above-chance guesses or hunches when it came to criminal matters was extrasensory is less important at this juncture than establishing that the ability actually exists" (Truzzi, 1990-1991, p. 2).

Nickell as false skeptics who are actually deniers rather than doubters (Lyons & Truzzi, 1991, pp. 131-132). Though we clearly indicated that psi remained unproved rather than disproved, for "true disbelievers" like Baker and Nickell a position of "undecided" seems intolerable. It must therefore be classified as too friendly to the opposition and thus located within the believers' camp. For those like Nickell and Baker, there seems to be no room for any middle ground. This mindset permeates much of Nickell's book, and it is a perspective that not only distorts the position of opponents but also produces blindspots that made Nickell and his collaborators neglect to apply the same critical analysis used against opponents to works congruent with their own biases.

Though Nickell and his contributors have apparently read *The Blue Sense* (for they copiously cite it), they have failed remarkably to respond to almost all our criticisms of past debunkings, and they here treat such debunkings as accepted and uncontroversial. They liked and even commended us when we joined in their debunking of the psychics, but they seemed blind to most points we raised about errors in past criticisms, and they repeat some of these errors here. For example, in the book's appendices there are reports of two experimental studies by Martin Reiser and his Los Angeles Police Department colleagues, who claim that their findings discredit the psychics tested. We criticized these studies on several grounds, but the most important was the following:

Only 50 percent of the information provided by the psychics was deemed verifiable and was actually included in the final analysis. The information considered unverifiable included such things as statements about "accessories to the crime, lifestyle of the victim and/or suspect, and psychological traits of the victim and/or suspect." Clearly, such statements might not lend themselves to *easy* verification, but they are not unfalsifiable and certainly could be useful toward the solution of a crime should they be true. But even for those statements treated as verifiable, these experimenters failed to distinguish between cases where the psychic provided incorrect information and those where no information at all was provided. Silence may not be a hit, but the study treats it the same as if it were a miss. This makes its statistical analysis questionable. (Lyons & Truzzi, 1991, pp. 52-53)

Nickell ignores this methodological problem present in both of Reiser's studies. Instead, he raises the point (made to him by Jim Lippard) that the data might be interpreted as showing that the students in the control group were "actually more psychic than psychics themselves!" (p. 14). Nickell seems to think this is a clever point of rebuttal; but in fact I agree with him. If proper statistical analysis were to show significantly higher scores for the controls (students and detectives) than for the "psychics," this result would suggest either that the students

and detectives were more psychic or that the "psychics" were psi-missing. Since one may question the ability of the "psychics" that Reiser used (they were not individuals either with past police endorsements or with demonstrably good track records), it seems more likely that the control groups may have had more of the "blue sense" than Nickell presumes. If Reiser still has the raw data, a reanalysis in light of the above criticisms and alternative hypotheses might prove useful.

Having commented on Nickell's introduction and the book's general orientation, let me briefly comment on each of the contributed articles. The essays by Henry Gordon and by Stephen Peterson on the late Dutch paragnosts Peter Hurkos and Gerard Croiset were disappointing in that they presented largely derivative reviews. By demolishing targets previously destroyed, they chose relatively easy targets, and they presented little, if any, new information about their subjects. Michael R. Dennett's essay on Dorothy Allison, on the other hand, was excellent in bringing much new information forward, almost all to Allison's discredit. Since Allison reportedly has carefully kept files on all her now probably thousands of cases (she is thus exceptional among psychic detectives), and since she has indicated a willingness to allow some investigators to examine these, it is a shame that Dennett was unable to obtain Allison's cooperation. Impressed as I was by Dennett's essay, a comprehensive analysis of Allison's work remains badly needed.

Gary P. Posner's essay on Noreen Renier is perhaps the most vitriolic in the book. Posner has been Renier's public antagonist for some years now, and his analysis is far from dispassionate. His antagonism is related to Renier's having won a lawsuit for libel against John Merrell, a once prominent member of the Northwest Skeptics whom Posner defends. (From Posner's account, one might wonder why Merrell ever lost the case.) What Posner, like many critics, apparently fails to understand is that professional psychics like Renier may refuse to cooperate with an investigator, not because they are afraid to have their abilities tested, but because they simply do not trust the honesty and integrity of their challenger. Posner, like James Randi, likes to throw down the gauntlet and then assert that the challenged psychic has an obligation to cooperate with him. Since I have been in regular touch with Renier for some years now and have found her highly cooperative in answering my inquiries (and she knows that I am not convinced that she is psychic), and since I have also heard her complain about the inquisitorial tactics of some Florida skeptics, I am not surprised that she has ignored Posner's demands for responses to his charges. In Posner's critique, he nit-picks through materials searching for anything compromising, much like a prosecuting attorney. He construes all that he can in a light unfavorable to Renier. Thus, for example, he suggests that Renier's

having a promotional packet contradicts her statement that she has not advertised for or solicited police case work (p. 67). He disregards the fact that her promotional packet is used to obtain lecture and speaking engagements, not police cases (which do, in fact, come to her unsolicited). Similarly, Posner notes that Renier says she "will not accept a case unless an officially authorized representative of the agency having jurisdiction contacts her directly," and he then claims that this contradicts ex-FBI agent Robert K. Ressler's statement that Renier had not been used by the FBI in "any regular capacity" (p. 69). Posner ignores the obvious distinction between formal and informal solicitation of her services; the fact remains that FBI agent Ressler asked for her help on a case. Yet Posner also pounces on small and petty distinctions. For example, Ressler said that Renier was not an instructor for the FBI (p. 69). This is correct in that she has never had a regular position with that title there, but Posner overlooks the fact that Renier had been an invited lecturer at the FBI Academy. Although Posner does provide some new information and raises some interesting questions, his adversarial and one-sided approach leaves one more frustrated than enlightened.

Jim Lippard's essay on Bill Ward is, in my view, the most judicious and fair-minded appraisal in the book. In exemplary fashion Lippard concludes his analysis by simply and moderately saying that "the case for Bill Ward's psychic abilities remains at best unproved, and certainly does not support his own claims of success and accuracy" (pp. 97-98). Lippard's essay strikes me as being the main contribution to this book that is clearly out for truth rather than blood.

The examination of Rosemarie Kerr by Lee Roger Taylor, Jr., and Michael R. Dennett is limited to a single case involving her, one made prominent by its inclusion in a TV documentary. This limitation occurred largely because of Kerr's lack of cooperation with the authors about other cases. Though they raise some interesting points, their arguments often include extreme speculations. Worse, they attribute guilt where there is merely a lack of cooperation with them by the psychic, the victim's family, and the television producers. (This verdict might be termed "guilt by nonassociation.") Yet it is arrogant if they presume that Kerr and her advocates are obliged to deal with what they probably view as hostile and self-appointed interrogators.

Like the chapter about Kerr, the essay by Kenneth L. Feder and Michael Alan Park on Phil Jordan is really about a single case; but, like Lippard's contribution, it is well done. It raises some excellent new points and is presented relatively dispassionately. My only complaint is that the authors assert that Lyons and I made several errors (which we may have done in our very brief discussion of Jordan's case), and they imply that these mistakes originated with us. In fact, we cited all the

sources for our "facts" on this case; but since Feder and Park do not include those sources in their bibliographical notes, one wonders whether they consulted them.

Ward Lucas largely repeats here (with some updating) his previously published analysis of Greta Alexander. Lyons and I admired but also criticized this earlier analysis. Like Nickell, Lucas misrepresents our position and ignores our methodological criticism of his earlier analyses. He quotes our conclusion that "use of the blue sense [psychic ability] may only give us a . . . small advantage [in crime solving] but it perhaps remains better than nothing" (p. 131, brackets those of Lucas); but his insertion of "psychic ability" in brackets—thus equating the blue sense with psi, as I noted earlier—is flatly wrong. He also takes out of context my interview statement about Alexander that "she seems to have an unusually good track record in terms of finding bodies in water . . . I am comfortable in recommending Greta because I don't feel she is going to rip anybody off" (p. 131). Lucas says that this statement shows that I take "an even more generous stance toward the relationship between Greta Alexander and law enforcement" (p. 131). I would have thought it obvious that I am saying nothing here about her relationship with law enforcement, for my reference here is to my view of her character and not her alleged powers. My big problems with Lucas's analysis, however, are that he examines only a very few of her great many cases endorsed by police, and that he does not deal with our criticisms of his analysis of the Cousett case, for which Lucas offers a series of usually plausible but still improbable counterexplanations (given the number of alleged hits). Lucas's hostility toward psychic detection and his willingness to accept extreme counterexplanations may stem from his extremist view of its implications, for he asserts that if a psychic actually led the police to a body by the use of such powers, this would be a "miracle . . . that overturned all the laws of logic, reason, science, physics, and probability" (p. 150). Certainly most psi proponents do not espouse so revolutionary a view. Lucas also harshly insists that the use of psychics is far from benign, that "it is negative, and adversely impacts the public good" (p. 154). His study of Greta Alexander hardly demonstrates such a negative impact, especially since even Lucas acknowledges (p. 135) that "the side of Alexander's persona that has endeared her to the people of Southern Illinois is her charity," which includes her "House of Hope" for needy families, which she supports from income she derives from her psychic consultations.

The book's penultimate chapter, by Nickell, briefly considers several psi sleuths not covered earlier in the book, including Robert James Lees, Doris Stokes, Nella Jones, the late John Catchings, Marinus Dykshoorn, Brett Cardorette, Etta Louise Smith, Normand Joyal, Armand Marcotte,

Verne McGuire, Ann Gehman, John Monti, and William J. Finch. Nickell scores some good negative points, but most of these are already in the literature (which he cites). The problem with this chapter is its pot-shot approach. The author fails to consider other cases by the psychics he covers, and there is also a lack of any systematic selection of those he does cover. Thus, Nickell gives attention to some psychics who have made the papers with only one case, whereas others with numerous cases purportedly to their credit are entirely absent (for example, Nancy Czetli, Kathlyn Rhea, and Gene Dennis). This criticism can also be applied to Nickell's whole book, for he and his colleagues never give any basis for their selection of these particular psychics out of the list of several hundred possible psychics (my own files list at least 350 psychics who have been involved with police cases).

The final chapter by James Alcock nicely summarizes many of the factors involved in people's acceptance of the claims of psychic sleuths. He even includes a 9-point "how to" course on becoming a pseudopsychic sleuth. I agree with most of Alcock's observations, especially his invoking what he terms "[Ray] Hyman's Categorical Imperative," which asserts that we should not try to explain something before we are sure there is something that needs an explanation in the first place. Oddly, there are several cases in this book where the writers ignore this dictum. For example, Lucas tries to explain away Alexander's predictions, even though he also acknowledges that there is some question as to exactly what those predictions really were. The main trouble, though, is that Alcock's skepticism remains one-sided. The psychological mechanisms that bias us toward undue belief may also act to bias us toward undue disbelief. Alcock (and the other contributors) complain about anecdotal evidence as being poor when it is used to support psychic claims, but they leap unskeptically toward acceptance of similar anecdotal reports when they are used to discredit such claims. Critics are too often uncritical toward their own supporters.

The first four of the book's five appendices demonstrate this last point in a particularly strong way. This is highly ironic since these four studies represent the critics' most controlled efforts at scientific examination, yet they err in ways that surely would have been attacked by Nickell et al. if their empirical findings had gone in an opposite direction. I have already indicated that Appendices A and B (the Reiser et al. experiments) have methodological problems ignored by fellow critics. Since my criticisms of these data analyses are elementary, I am surprised that the first study, originally published in a criminology journal, ever passed a peer review. (Reiser et al.'s second study, originally published in Reiser's book, seemingly did not have to undergo a refereeing process.)

The second two appendices make my point even better. Appendix C is an article by Jane Ayers Sweat and Mark W. Durm (reprinted from *The Skeptical Inquirer*) that centers on their polling of urban police departments. Their survey revealed that 35% of the 50 polled departments had at some point tried using a psychic (though most also report that they did not find the psychics useful). There are numerous problems with this survey. Sweat and Durm discount any "underrater bias' among respondents since identification with psychics among police could have negative connotations" because of what Sweat and Durm view as "the conviction with which the comments were made" (p. 216). This seems hardly justified. In fact, the strength of the negative comments by the denying respondents can be viewed as indicating exactly the opposite. Anyone who is familiar with the literature or who has done any interviewing of psychics and of the police who have used them should know that in the vast majority of instances, police have used psychics informally or unofficially; they have usually been used by someone working the case, and not through formal or departmental instigation. In fact, my files contain many news stories of psychic detection cases, including even interviews with the police involved, in several cities whose departments had told Sweat and Durm that they had never used a psychic. In light of all this (and I here overlook what I think is the slant of their survey questions toward eliciting negative responses), an acknowledgment by 35% of these urban departments that they officially tried using a psychic is surprisingly high.

Because their study was criticized for polling urban departments even though most psychic detectives reportedly have worked on rural cases (Galde, 1993; Truzzi, 1993), Durm and Sweat conducted a second study (Appendix D) in which they polled the police departments of 75 small and 75 medium-sized cities. They found that only 19% of the departments of small cities and only 28% of the departments of medium-sized cities said that they had tried using a psychic. They conclude that this study has empirically shown Galde and Truzzi to be incorrect because "rural police departments use psychics less than large cities" (p. 232). Durm and Sweat apparently base this conclusion on their simple comparison of percentages (35% for urban cities, as opposed to 28% for middle-sized cities and 19% for small cities). Since the comparison is of percentages of different sets rather than of the actual numbers in their own data, Durm and Sweat completely miss the point of criticism, which was that psychics are more frequently used by rural than by urban departments. Just because the percentage of rural departments reporting the use of psychics was lower, this figure does not mean that they used fewer psychics. The problem with their analysis should be obvious (although it seems not to have been obvious either to them or to Nickell).

The urban cities that they polled were the "50 largest U.S. cities" (p. 215); the population of the smallest cities ranged from "31,092 to 33,181" (p. 225), and that of the medium-sized cities ranged "from 50,889 to 55,097" (p. 225). Whereas all 50 of the urban departments responded (two declining to answer the question), only 43% of the departments of the small and medium-sized cities responded. Durm and Sweat assume that any response bias must have been in favor of, rather than against, the use of psychics (p. 227), but this is easily contested, especially if they are wrong in their belief that "the wording was not biased" (p. 225) in their questionnaire. Even if we ignore such secondary criticisms, however, their primary and elementary error remains. Let us for the moment assume that their results are valid as derived. The fact is that there are *at the very least* 10 times as many small and medium-sized cities as there are the 50 largest ones. Thus, although the percentages for small and medium-sized cities using psychics are lower than for urban ones, the actual figures are not. Durm and Sweat received answers from the departments of 48 large cities, 32 small cities, and 33 medium-sized cities. This total of 65 small and medium-sized cities, or a 43% response rate (p. 225), concurs with the data presented in Tables 1 and 2 (pp. 230, 233).² If we assume that these 65 cities are representative of small and medium-sized cities, as Durm and Sweat believe, and if there are only 10 times as many cities of this size as there are the largest cities they examined (in fact, there are substantially more than this), we need to multiply by 10 the total number of small and medium-sized cities using psychics (which is 6 plus 9, or 15). This gives us a total of 150 small and medium-sized cities, compared to 17 large cities that said they have used psychics. Obviously, then, Durm and Sweat's critics are right in asserting that more rural than urban departments have tried psychics. Durm and Sweat's own data contradict their argument. Aside from all this, their really important and unappreciated finding is that the numbers derived by Durm and Sweat—which reflect officially acknowledged cases and do not include the unofficial and informal uses that probably cover around 80% of the psychics' cases—are much higher than most previous analyses would have led us to expect.

The final Appendix (E) reprints a generally reasonable article by Walter F. Rowe. He found Lyons's and my book "ultimately unsuccessful" but endorsed our comment that "the data are simply inadequate for the

² Table 3 is a mislabelled "Summary of Data of Small-Sized, Medium-Sized Cities in America," because the authors elsewhere (p. 232) indicate that it is a "summary of both studies," thus including the largest cities as well. My discussion here refers only to the figures given in their text, ignoring the strangely combined data in Table 3 which, as I here argue, produce quite inappropriate and misleading aggregated percentages.

refined analysis we need" (p. 243). Until we get adequate data, perhaps any attempt at a comprehensive survey must remain "ultimately unsuccessful." Alas, Nickell's book does little more to move us toward that still needed refined analysis.

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MARCELLO TRUZZI

Department of Sociology
Eastern Michigan University
Ypsilanti, MI 48197

SEVEN EXPERIMENTS THAT COULD CHANGE THE WORLD by Rupert Sheldrake. London: Fourth Estate, 1994. Pp. xi + 269. £15.99 (hard-cover). ISBN 1-85702-167-3.

The premise of *Seven Experiments That Could Change The World* by Rupert Sheldrake is that scientific endeavor should not be the sole jurisdiction of men in white coats tucked away in research laboratories. Sheldrake feels that it is possible for amateur scientists from all walks of life who are curious about the world around them to make discoveries that push back the current boundaries of knowledge. His reasoning is that institutional science has a tendency to ignore areas of research that may throw light on apparently anomalous phenomena. Therefore, in this "do-it-yourself guide to revolutionary science," the author attempts to persuade readers to take part in scientific discovery. Accordingly, he provides the background and methods for the average layperson to investigate seven questions that he feels have no satisfactory answers at present.

In Part One, Sheldrake looks at "the extraordinary powers of ordinary animals." Many pet owners will testify that their pets have uncanny abilities of one sort or another, and Sheldrake refers to a number of accounts in which dogs stop whatever they are doing and go to meet the often unexpected arrival of their owner. Possible cues from others at home with the dog are often ruled out on occasions when the dog's owner returned at an unusual time. Of course, the author recognizes that these

CORRESPONDENCE

To the Editor:¹

Throughout our exchange of letters I have done my best to pin down the shifting views of Marcello Truzzi. While he (with his co-author) advocates "the continued cooperation of both police and psychics" (an affirmative position), he resists my (affirmative) label "promotor of psychics," now styling himself as merely one "who does not condemn" psychic claimants (a neutral position).

In his previous letter he insists that Uri Geller was never caught faking anything, despite "the lens cap episode reported in *Popular Photography*," but he now concedes, "I personally do believe it [i.e., a photo in *Popular Photography*] shows his [Geller's] fingers holding the cap." This is an experiment in which Geller was supposed to project his ballyhooed "mental powers" through a lens cap that was ineffectively taped in place. Yet Truzzi still insists, with characteristic perversity, "The evidence against Geller in this instance is not so unambiguous as Nickell imagines." (Hmmm. Maybe Mark Fuhrman planted the lens cap in Geller's open hand, or possibly the photographer who took the photo imagined Geller holding the cap and thus thoughtgraphically projected that image onto the film, or . . .)

Again, Truzzi previously insisted that his view of his former association with CSICOP "is documented in the minutes of past meetings of CSICOP's Executive Council (which Nickell could consult)"; yet when it turned out I had consulted the minutes and there was no such documentation for his assertions, Truzzi took a different tack: "Since the minutes are silent on these matters, they disconfirm nothing I wrote."

Yet again, while in his review of my *Psychic Sleuths* (JP 58: 432-441)—which began this exchange of letters—Truzzi faulted skeptics for being dismissive of psychics when the skeptics "are unable to actually disprove" psychic claims. Truzzi still denies he has done what he has obviously done, attempted to shift the burden of proof, and now discourses philosophically upon nonproof versus disproof as if that were the issue and I did not understand the difference. In fact, I can even recognize the attempt to disguise one as the other. Note it is Truzzi who suggests

¹ The following letters from Joe Nickell and Marcello Truzzi are a continuation of their correspondence that appeared in previous issues of the *Journal* (September, 1995, pp. 283-285; June, 1996, pp. 185-190; September, 1996, pp. 278-281).

skeptics must *disprove* a claim. And as I indicated in an earlier letter (June 6, 1996), Truzzi continues to defend Uri Geller on the grounds that (supposedly) skeptics have not sufficiently debunked him.

It seems to me that Truzzi shifts his view when it suits him, always making it look like there is a misunderstanding that his opponent is responsible for. This may be a necessary tactic for one who finds it comfortable sitting on the fence.

JOE NICKELL

1992 Sheridan Drive, Suite 6
Buffalo, NY 14223

To the Editor:

Based on his continued misrepresentation of my quite constant position which he characterizes as "shifting," I can only urge readers to examine my previous replies to Dr. Nickell with greater attention than he seems to have given them. Perhaps the main benefit of these exchanges has been what it reveals about the dogmatism of Nickell's views, for he consistently sees things in simple black and white terms. Thus, he misinterprets my statement calling for continued cooperation of both police and psychics with researchers (not with each other) as an "affirmative position" that he contends promotes psychics and so goes beyond my "neutral position" of mere failure to condemn psychic claimants. Clearly, anything less than condemning psychics is interpreted by Nickell as promoting them.

Similarly, he confuses my personal belief that Geller has cheated with what I would consider to be scientific proof that he has. Nickell apparently does not understand the fundamental difference between belief and knowledge. In any case, the cited *Popular Photography* article itself made the judgement that the evidence against Geller in that episode was inconclusive and called for another and better-controlled test.

Though I mistakenly (perhaps naively) expected the details of my resignations at CSICOP to be in the minutes (which, of course, were not available to me after I left CSICOP), those details are still documented elsewhere, and Nickell surely could have gotten his facts straight on these details from numerous sources including some of his colleagues at CSICOP.

Finally, Nickell continues to show he simply does not understand that the burden of proof in science is on any claimant whether the claim be positive *or* negative. A conclusion of nonproof (true skepticism) carries

no burden, but a claim of disproof does. According to the rules of science, it is Nickell who shifts the burden of proof to the critic when he goes beyond concluding there is nonproof of someone's affirmative claim and instead makes an assertion of disproof (a negative claim of denial and not merely a skeptical claim of doubt).

MARCELLO TRUZZI

*Department of Sociology
Eastern Michigan University
Ypsilanti, Michigan 48197*